Is There Too Little Antitrust Enforcement in the US Hospital Sector?*

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Abstract

From 2002 to 2020, there were over 1,000 hospital mergers in the US. During this period, the Federal Trade Commission (FTC) took enforcement actions against 13 transactions. We show that, from 2010 to 2015, 20% of hospital mergers could have been predicted *ex ante* to meaningfully lessen competition and find that these mergers resulted in price increases of over 5%. The five-year present discounted value of preventing a year of hospital mergers is, on average, $968 million, which is larger than the entirety of the FTC's budget. Ultimately, mergers with larger price increases tended to be in less affluent regions.

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1 Introduction

The two federal agencies that engage in antitrust enforcement in the United States (US) — the Federal Trade Commission (FTC) and the Department of Justice (DOJ) — play a vital role in preserving competition across the economy by enforcing federal antitrust laws that prevent the creation of market power through mergers. However, over the last 20 years, rising concentration across US industries has fueled concerns that federal antitrust laws are underenforced (Kwoka, 2013; Baer et al., 2020). For example, from 2000 to 2020, antitrust agencies only took enforcement action to block 2% to 3% of all mergers (Kades, 2019).

While this pattern is striking, this permissive approach to enforcement could theoretically arise if the mergers occurring pose little threat to competition. Enforcement is costly to agencies, so they may opt only to take action against mergers that are clearly anticompetitive. Alternatively, antitrust enforcement may be inefficiently low because of external impediments. Critics of the current antitrust paradigm have pointed to many such impediments, including low enforcement budgets, weak reporting requirements for merging parties, and legal precedents that favor merging parties over the FTC and DOJ (Wollmann, 2019; Baer et al., 2020; Gaynor, 2021).

In this paper, we evaluate whether there is too little antitrust enforcement in the US hospital sector, a $1.2 trillion industry (6% of US Gross Domestic Product (GDP)) where there has been particular concern about lax antitrust enforcement (Dafny, 2021; Gaynor, 2021). From 2002 to 2020, there were over 1,000 mergers among the nation's approximately 5,000 short-term general acute care hospitals. During this period, the FTC (the enforcement agency which investigates hospital mergers) only took action to block 13 deals — an enforcement rate of approximately 1%.[1] Partly as a function of this consolidation, at present, 90% of US metropolitan areas have hospital markets with a Herfindahl-Hirschman Index (HHI) of over 2,500 points, making them "highly concentrated" according to the 2010 DOJ/FTC Horizontal Merger Guidelines (Fulton, 2017; U.S. Department of Justice and Federal Trade Commission, 2010).

If the FTC is optimally targeting enforcement, then mergers that they do not challenge should have minimal effects on competition and prices. As a result, a simple test of the efficacy of antitrust enforcement entails examining whether there are consummated mergers occurring that could have been predicted *ex ante* to lessen competition and which, *ex post*, raised prices.

We carry out this test by analyzing hospital mergers in the US using insurance claims data from three of the five largest insurers in the US — Aetna, Humana, and UnitedHealthcare — provided by the Health Care Cost Institute (HCCI). We estimate the post-merger price increases generated by 322 horizontal hospital mergers involving 702 hospitals that occurred between 2010 and 2015. Of

[1] Enforcement actions are defined as matters that resulted in a final consent order requiring divestitures, matters where the parties abandoned or restructured the deal as a result of antitrust concerns raised during the investigation, or matters in which the FTC initiated litigation to block or undo the merger.

note, outsiders view the FTC as being particularly active in hospital antitrust enforcement during this period (Capps et al., 2019). We estimate that the average merging hospital raised prices by 1.6%, with of 1.1% for inpatient care and 1.8% for outpatient care.

Collectively, as we illustrate, these mergers raised health spending by hundreds of millions of dollars, and their aggregate effect relative to the FTC's budget is striking. We estimate that an average year of mergers during this period raised hospital spending on the privately insured *in the first year following the merger alone* by $204 million. Moreover, since post-merger price increases are persistent, these spending increases add up over time. The present discounted value of the spending reduction from blocking a year of hospital mergers (on average 53 mergers) over the next five years is $968 million. For context, the FTC's average annual overall budget and antitrust enforcement budget between 2010 and 2015 were $315 million and $136 million, respectively.[2] As a result, the five-year spending increases generated by a year of hospital mergers — a sector of the economy that accounts for 6% of GDP — were three times larger than the entirety of the FTC's budget.

Are the mergers that led to large price increases the ones that the FTC could have *ex ante* predicted to be harmful via a lessening of competition? To answer this question, we use two common pre-merger evaluation methods to flag presumptively anticompetitive mergers and analyze whether they generated differentially large price increases. First, we flag mergers using cutoff rules for post-merger changes in HHI defined by the DOJ/FTC Horizontal Merger Guidelines as likely to harm competition. Our flag follows the Horizontal Merger Guidelines, which note that mergers that result in increases in HHI of at least 200 points and lead to a post-merger HHI of over 2,500 should be "presumed to be likely to enhance market power" (U.S. Department of Justice and Federal Trade Commission, 2010). Second, we flag mergers based on whether the merging parties experience increases in willingness-to-pay (WTP) of 5% or more (Capps et al., 2003; Garmon, 2017; Raval et al., 2017). WTP is the dominant approach used in hospital antitrust enforcement actions for *ex ante* prediction of merger-driven price increases (Dranove and Ody, 2016; Capps et al., 2019).

While the average hospital merger raised prices by 1.6%, we show that this average estimate masks important variation in the post-merger price increases across transactions. We find that approximately 20% of consummated transactions could be predicted *ex ante* to increase concentration or lessen competition via our flags for the changes in HHI or WTP. The flagged transactions in our sample generated differentially large price increases relative to deals we did not predict would run afoul of the Guidelines. Likewise, transactions that we flagged under the Horizontal Merger Guidelines' HHI cutoffs increased the merging parties' prices by 5.2% via increases in inpatient and outpatient prices of 5.4% and 4.5% respectively. Transactions that generated WTP increases of over

[2] These budget figures are drawn from the annual reports of the FTC's Congressional Budget Justifications and presented in 2017 dollars.

5% raised overall hospital prices by 3.6% after the mergers were consummated, with large inpatient price increases (4.6%) and imprecise outpatient price increases. Ultimately, the existence of a substantial number of presumptively anticompetitive transactions with large *ex post* price increases provides evidence of potential underenforcement.

While hospital mergers occur across the US, their burden is felt unevenly. We estimate that post-merger price increases are larger in areas with higher poverty rates (3.2% vs. 0.4%), areas with lower wages per capita (2.5% vs. 1.5%), and areas with lower population density (8.9% vs. 1.4%). We posit that these results stem from two factors. First, given the FTC's limited budget, the agency may be prioritizing investigating transactions involving larger merging parties, which tend to be in more densely populated, wealthier regions. Second, antitrust enforcement typically focuses on the market for inpatient services, not the market for outpatient services (Capps et al., 2019). Poorer, less densely populated regions tend to have thinner markets for outpatient services. Consequently, as we illustrate, mergers in these areas generated outsized increases in outpatient prices.

Our analysis has several limitations. First, we do not measure whether these mergers impacted quality. However, past academic work has not found that mergers raise quality, and a broader literature highlights that, when hospitals become exposed to competition, they tend to raise their clinical quality (Beaulieu et al., 2020; Cooper et al., 2011; Gaynor et al., 2013). Second, we are also not able to assess the effect of mergers on hospital efficiency (i.e., lower costs). However, a growing literature (e.g., Craig et al. (2021)) has found that hospital mergers do not meaningfully lower costs. Moreover, if efficiency improvements exist, we find that they are not being passed through, on average, into lower prices. Third, we focus on *consummated* mergers, which are less likely to have large *ex post* price increases than mergers that were successfully blocked or otherwise preempted by existing regulatory practice. As a result, our analysis should not be used to predict the effect of future mergers that are being proposed.

This study joins a growing merger retrospectives literature which has assessed deals across many industries (Ashenfelter and Hosken, 2010; Ashenfelter et al., 2013, 2015; Miller and Weinberg, 2017). Closest to our work outside of the hospital industry are Bhattacharya et al. (2023) and Majerovitz and Yu (2021), who perform large-scale merger retrospectives in the consumer packaged goods industry. Consistent with our results, both groups find that the average merger modestly increases prices, with substantial variation across transactions.

We also contribute to a recent literature analyzing the effect of hospital mergers on prices (Dafny, 2009; Haas-Wilson and Garmon, 2011; Garmon, 2017; Cooper et al., 2019; Brand et al., 2022). Consistent with this literature, we find that the average hospital merger raises prices. We expand on this literature in four ways. First, we highlight that the mergers with the largest price increases are those that could have been predicted *ex ante* to lessen competition and include those that ran afoul of the Horizontal Merger Guidelines. Second, we show that while the FTC is intervening in the most anticompetitive transactions, there are many transactions which run afoul of the Horizontal Merger Guidelines and meaningfully lessen competition where the FTC is not taking action. Third, we find that post-merger price increases are larger in less affluent areas of the US. Fourth, in contrast to the prior literature, which has primarily focused on inpatient care (which is where regulators focus their attention), we show that mergers generate price increases for outpatient services that are at least as large as inpatient price increases.

2 Data and Measurement

2.1 Measuring Hospital Prices

To measure hospital prices, we leverage data from HCCI. The HCCI database includes the near universe of health insurance claims for insurance plans offered by Aetna, Humana, and United-Healthcare between 2008 and 2017. We focus on individuals with employer-sponsored coverage who are under 65 and for whom an HCCI payor is their primary insurer. The HCCI payors cover approximately 28% of individuals with employer-sponsored health insurance (Cooper et al., 2019). Crucially, these data contain the negotiated transaction price — or "allowed amounts" — for each service that was provided.

Hospitals are multi-product firms that offer numerous services, each with their own price. Hospitals differ in the mix of services they offer and the demographic profile of the patients they treat. Therefore, following Cooper et al. (2019) and Gowrisankaran et al. (2015), we construct an adjusted "price index" to summarize the average price level for each hospital-year in our data. We do so separately for inpatient and outpatient services. Specifically, we estimate two regressions of the form:

$$\log(p_{idht}) = \alpha_{ht} + \beta X_i + \pi_{dt} + \varepsilon_{idht}, \tag{1}$$

where the price of case i of type d (diagnosis for inpatient services; procedure for outpatient services) at hospital h in year t is a log-linear function of a hospital-year fixed effect α_{ht}, controls for each patient's age and gender X_i, and type-year fixed effects π_{dt}.

We then use the estimates from Equation (1) to generate predicted values for each hospital-year, holding fixed the coefficients accounting for patient characteristics and clinical severity at the average levels in the data ($\overline{X}$ and $\overline{dt}$ respectively):

$$p_{ht}^{INDEX} = \hat{\alpha}_{ht} + \hat{\beta}\overline{X} + \hat{\pi}_{dt}\overline{dt}. \tag{2}$$

where p_{ht}^{INDEX} is the estimated price index for a hospital in a given year. For some analyses, we present results using a "composite" price index that represents a weighted average of our inpatient and outpatient price indices according to the hospitals' share of revenue that comes from inpatient and outpatient services respectively.

For our inpatient price index, we use Diagnosis Related Group (DRG) codes to define the π_{dt} fixed effect. For the outpatient price index, we use the Current Procedural Terminology (CPT) procedure code that maps to a valid Medicare Ambulatory Payment Classification (APC) payment rate to define the π_{dt} fixed effect. Outpatient visits can involve a number of procedures. To ensure the prices we measure cover all the services rendered during a visit — not payments negotiated as a bundle of services — we limit our analysis to outpatient cases for which the patient has no other outpatient cases on the same day. Although this restriction limits the data to approximately 30% of patient days, we view this sample as one that provides a clean distinction between price and quantity.

2.2 Hospital Ownership Transitions

The primary data we use to measure merger activity come from the American Hospital Association's Annual Survey of Hospitals (AHA). These data contain biographical information on the near universe of general acute care hospitals hospitals in the US, including a measure of system ownership. Our final roster contains 4,846 hospitals. We track mergers and acquisitions in our hospital panel using changes to the system identifier provided by the AHA for 2002 to 2020. We leverage several additional data sources — the FactSet Research Systems database, the Irving Levin Associates' Health Care Services Acquisition Reports, and the Securities Data Company Platinum — to verify the existence and timing of mergers.[3]

Along with data on mergers, we collect data on pre-merger notification and enforcement activity from the FTC. We observe the yearly counts of pre-merger notification filings and enforcement actions undertaken as reported in the FTC's Annual Reports to Congress Pursuant to the Hart Scott Rodino Act.[4] We restrict our focus to cases with reported NAICS codes starting in "622," which indicate the acquired firms are hospitals. Because this category includes hospital types we do not study, as well as acquisitions of hospitals by non-hospital entities, these reported figures should be considered an upper bound on relevant activities.

We plot all mergers in our database from 2002 to 2020 in Figure 1. We observe 1,164 mergers of general acute care hospitals. Notably, only 465 (40%) transactions were reported to the FTC during this period per the HSR Act reporting requirements. This suggests that more than half of hospital mergers fall below the HSR Act's reporting thresholds because of the revenue of the merging parties. Among consummated transactions, we estimate that 238 (20%) mergers involved

[3]For more information on the methodology we use to track hospital ownership, see Appendix D of Cooper et al. (2019). We expand the panel from that prior paper back to 2002 and forward to 2020.

[4]See `https://www.ftc.gov/policy/reports/annual-competition-reports`.

at least one party that experienced an increase in HHI of greater than or equal to 200 points, which resulted in a post-merger HHI of 2,500 points or greater.[5] Nevertheless, during this period, the FTC only engaged in enforcement actions to challenge 13 mergers. This implies that the agency challenged approximately 1% of all transactions and, at most, 5% of transactions that likely ran afoul of the thresholds set in the Horizontal Merger Guidelines.

3 Empirical Strategy

We estimate the causal effect of mergers using a difference-in-differences design. We follow the approach used in several prior studies (Cengiz et al., 2019; Brot-Goldberg et al., 2021; Craig et al., 2021) to address concerns about staggered timing (Roth et al., 2023). Our general approach is to construct an "experiment" containing one merging hospital and a "control" group of non-merging comparison hospitals. We then estimate average treatment effects by stacking these experiments and estimating separate unit and time fixed effects for each experiment group.

For this exercise, we build an "analytic" sample of mergers and focus on the set of hospitals that merged between 2010 and 2015 that were located within 50 miles of at least one hospital in another system. Particularly for large national systems, the 50-mile restriction allows us to focus on the subset of hospitals that are plausibly affected by the merger. We focus on the period from 2010 to 2015 because it aligns with the period where we can accurately measure hospital prices for at least two years before and two years after merger events using HCCI data. Our final sample contains 702 hospitals representing 322 transactions.[6] We map merging hospitals in our analytic sample in Appendix Figure A.3, highlighting the transactions we estimate would be flagged under the Horizontal Merger Guidelines.

In order to identify the treatment effect, we need a comparison, or "control," group of non-merging hospitals to form counterfactual trends in prices. Our control group is composed of hospitals that either did not experience a merger in our sample period (plus the two years leading up to it) or hospitals that merged in future periods, provided that these control hospitals do not merge until after two years following the focal hospital's merger year. To ensure that our control hospitals represent plausible counterfactuals, we use propensity scores to match comparison hospitals to "treated" hospitals on pre-merger observable characteristics. We use a probit regression to estimate the propensity scores and find the merging hospitals' 25 nearest neighbors that did not merge between 2008 and $t+2$, where t is the year that the focal hospital merged. We also impose a caliper restriction so that propensity scores of matched controls must be within 20% of a standard deviation from the focal merging hospital, even if this requires that the control group contain fewer than 25

[5] We describe our measurement of HHI in Section 5.

[6] In Appendix Table A.1, we compare our analytic sample to the sample of all mergers. Our analytic sample is broadly representative of all mergers meeting the 50-mile restriction.

hospitals. For additional detail on our matching approach, see Appendix B.

We exclude merging hospitals from our sample if they appear to be "failing" pre-merger. We identify "failing" firms by whether their bed utilization in the year before the merger is below the first percentile, measured using Medicare's Healthcare Cost Report Information System (HCRIS) data. The logic behind this restriction is twofold. First, acquisitions of failing hospitals may involve larger changes to management practices or cost structure, rather than changes to competition or bargaining leverage. Second, if these hospitals would have closed in the absence of a merger, any suitable non-merging control hospitals would have also closed and would therefore not provide any price observations in the post-merger period.

Ultimately, in our estimation strategy, each group of one merging hospital and its matched controls form an "experiment" around each merger event, e. For each merger, we limit our analysis to the period covering two years before and after the merger. We then estimate a regression of the form:

$$\log(p_{eht}^{INDEX}) = \lambda_{eh} \times \mathbb{1}\{\text{merged}\}_{eh} \times \mathbb{1}\{\text{post-merger}\}_t + \eta_{eh} + \kappa_{et} + \varepsilon_{eht} \tag{3}$$

The primary set of parameters to be estimated are λ_{eh}, each of which estimates the percent change in prices for hospital h due to merger e. Under this approach, we effectively estimate a separate difference-in-differences regression for each merger, for each merging hospital. To estimate an average treatment effect across mergers, we stack experiments, maintaining experiment-specific estimates of η_{eh} and κ_{et}. This pooled regression gives equal weight to each merging hospital.

4 The Average Effects of Hospital Mergers

We begin by estimating the model in Equation 3, pooling all 702 merging hospitals in our analytic sample as described in Section 2. The resulting estimates give us the average effect of mergers on hospitals' inpatient prices, outpatient prices, and composite prices (which is a weighted average of inpatient and outpatient prices). As we illustrate in Panel A of Table 1, the average merging hospital raised its overall price by 1.6% via a 1.1% increase in inpatient prices and a 1.8% increase in outpatient prices. We plot an event study of these estimates in Figure 2. Across all three price measures, we find no significant difference in price trends between merging and non-merging hospitals in the two years prior to the mergers occurring, but we find persistent differences in price in the two years following the mergers.

Post-merger price increases are generally thought to result from mergers increasing market power. However, hospital prices could also increase if mergers cause hospitals to expand their operations, thus raising their marginal costs. To test this, we estimate whether hospitals increased their inpatient volume after mergers. We present these results in Appendix Figure A.4 and find no evidence that hospitals increased their quantity provision post-merger.

Another possible concern is that our approach involves averaging many merger-specific estimates, each of which is estimated imprecisely over a small set of hospitals. MacKinnon and Webb (2020) discuss how this can lead researchers to over-reject null hypotheses. We therefore construct a placebo test in the spirit of randomization inference. For each match group we construct, we drop the merging hospital and randomly assign treatment status to one of the non-merging control hospitals. We then re-estimate Equation 3 to get the average "post-merger price increase" for this placebo group. The null hypothesis (that mergers have no average effect on price) is true by construction in this approach, since there is no actually treated unit. Performing this procedure many times (redrawing the placebo-treated hospital each time) simulates the distribution of estimates under the null hypothesis. We do so 1,000 times and plot the distribution of average effects for our composite price measure in Figure 3, which includes a red vertical line reflecting our actual estimated average merger price increase. Our composite price effect estimates are larger than 99.8% of the placebo estimates (equivalent to rejecting the null hypothesis in a two-sided hypothesis test with a p-value of 0.004).[7]

We also illustrate that our estimates are robust to various perturbations of the specific analytic choices we make. In Appendix Table A.2, we show that our estimates are not sensitive to alternative matching approaches, such as not restricting potential controls or using LASSO regularization to limit the characteristics used to generate propensity scores. In Appendix Table A.3, we expand to mergers where the merging parties are less than 400 miles apart (rather than 50) and show that this does not meaningfully shift our overall effect.

4.1 The Aggregate Spending Increases Generated by Hospital Mergers

To assess the scale of the harm these mergers produced, we measure the total impact mergers had on spending for the privately insured through their effects on price alone. For each merging hospital, we fix the total spending on that hospital among the privately insured in the year prior to the merger. We then multiply $t-1$ spending by λ_{eh}, the post-merger price increase for hospital h in merger e. We then sum over the merging hospitals to capture the effect of merger-driven price changes on spending in a given year, holding quantities of care fixed.[8]

For the period 2010-2015, the average year of hospital mergers — 53, on average, annually — increased spending on the privately insured by $204 million in the year after they occurred (i.e., a one-year effect). To put this number in perspective, consider that the average yearly budget allocated to the FTC for *all* antitrust enforcement between 2010 and 2015 was $136 million.[9] Note that our estimate only considers the effect on a *single year* of spending. If we consider a five-year time

[7] We include the distributions for inpatient and outpatient prices in Appendix Figure A.5.

[8] We measure spending on the privately insured using data from Medicare's HCRIS data. For more detail on our approach to estimating aggregate spending changes, see Appendix C.

[9] These budget figures are drawn from annual reports of the FTC's Congressional Budget Justifications.

horizon with constant price effects, at a discount rate of 2.65%, the present discounted value of preventing a year of hospital mergers was, on average, $968 million.[10]

5 Treatment Effects for Mergers Predicted to Lessen Competition

The average merger in our sample raised hospital prices by 1.6%. In this section, we test whether certain mergers could have been predicted, *ex ante*, to generate above-average price increases via a lessening of competition using standard screening methods used by the FTC.

5.1 Changes in Concentration

The Horizontal Merger Guidelines note that mergers that result in post-merger increases in HHI of at least 200 points with a post-merger HHI of at least 2,500 should be considered presumptively anticompetitive. As a result, we flag mergers in our sample that would have generated HHI changes that would have been flagged using these standards.

To measure HHI, assume that a market M includes many hospital systems $S \in \mathscr{S}(M)$, where $\mathscr{S}(M)$ is the set of systems in M. Each system is defined as a set of one or more hospitals h, which have a collective owner. Formally,

$$HHI_M = 10,000 \times \sum_{S \in \mathscr{S}(M)} \left(\sum_{h \in S} s_{hM} \right)^2$$

where s_{hM} is h's market share within M. A fully monopolized market has an HHI of 10,000; if instead there are many small independent hospitals, the HHI will be closer to 0.

Measuring HHIs requires us to define relevant geographic markets and measure hospitals' market shares. We assume that a hospital's relevant market includes every hospital within a 30-minute drive time from their facility. We measure a hospital's market share as its share of inpatient hospital beds. We use hospital beds rather than activity to define concentration because, unlike hospital activity, changes in bed volume in the short run are unlikely to be highly correlated with changes in hospital quality or prices. We measure the change in HHI for a hospital h due to merger e, ΔHHI_{eh}, as the difference between the HHI in its market in the year before the merger and a computed counterfactual where we change system membership to reflect the merger, holding bed counts and the system membership of non-participating hospitals fixed. In Panel A of Appendix Figure A.1, we plot the distribution of ΔHHI_{eh}. The average merging hospital in our sample experienced an increase in HHI of 267 points.

We find that 82 of 322 (25%) transactions involving 109 hospitals generated an HHI increase of at least 200 points with a post-merger HHI of at least 2,500 and thus could have been flagged, *ex ante*, as presumptively enhancing market power, according to the Horizontal Merger Guidelines. In

[10] The 5-year US Treasury interest rate on January 4, 2010.

Panel B of Table 1, we find that these flagged mergers raised inpatient prices by 5.4% and outpatient prices by 4.5%. These increases are significantly greater than the price increases among mergers that did not result in such substantial increases in HHI.

There is not a well-established standard for market definitions, and market definitions are often an area of dispute in hospital merger cases (Capps et al., 2019). Therefore, in Appendix Table A.4, in addition to measuring the HHI in a market defined by a 30-minute drive time, we also present estimates where we define the market as a fixed 15-mile radius around the merging hospital. Although this alternative market definition generates different quantitative estimates, this result is robust using both measures of HHI.

5.2 Changes in Competition

WTP is one of the dominant screening tools used in hospital antitrust enforcement (Capps et al., 2019). In this section, we analyze whether mergers which WTP screening suggest would lessen competition resulted in larger *ex post* price increases. As Capps et al. (2003) and Gowrisankaran et al. (2015) note, patient demand for hospital care is quite inelastic to price. Therefore, the actors who discipline hospital prices are insurers, who negotiate with hospitals over prices directly. Insurers can obtain lower prices by credibly threatening to exclude the hospital from their network. The strength of this threat depends on consumers' *ex ante* WTP for the option to use the hospital in the event that they become sick. If WTP is lower, insurers can exert more leverage to lower prices. Under this model, hospital mergers raise prices because the insurer must exclude the *entire* merged entity if a deal is not struck, thus lowering the value of its plan offerings (Ho and Lee, 2017). These effects are greater when hospitals are closer substitutes. We provide further detail on the microfoundations of this measure in Appendix A.

We follow the literature and assume logit demand. Under this assumption, the WTP of patient i for system S is $\frac{1}{1-s_{iS}}$, where s_{iS} is the probability that i chooses a hospital in S. Our measure of the *percent* change in WTP is:

$$\Delta WTP_{eh} = \frac{\int_{i\in I}\left(\frac{1}{s_{i(S\cup S')}} - \left[\frac{1}{s_{i(S)}} + \frac{1}{s_{iS'}}\right]\right)}{\int_{i\in I}\left[\frac{1}{s_{i(S)}} + \frac{1}{s_{iS'}}\right]}$$

where S and S' are the systems participating in merger e, one of which contains hospital h.[11]

We estimate demand using our sample of inpatient admissions. We integrate over patients i within the set I, so that WTP for a given hospital is the sum of demand among relevant patients. As Capps et al. (2003) emphasize, patient heterogeneity in hospital demand and substitution is an

[11]We construct the *percent* change since the change in WTP we measure is proportional to the predicted price change.

important source of post-merger market power. We face a practical trade-off in accommodating heterogeneity. Flexibility improves the fit of the model. However, more flexible hospital choice probabilities — estimated using a smaller set of patients — are noisier. Following Raval et al. (2017), we take all hospitalizations in which a patient visited a hospital within 100 miles of their home zip code. We then partition the patients into groups g. We assume that, within-group, patients have the same (*ex ante*) preferences for hospitals, but we impose no restrictions on across-group differences. We assign groups based on patient observables (demographics, health, and location), then iteratively coarsen the partitions until they contain a minimum number of patients. Our primary specification uses a minimum group size of 50, resulting in 27,525 groups sized between 50 and 1,449. Given this setup, we can measure ΔWTP as above by replacing s_{iS} with its empirical analogue $\hat{s}_{g(i)S}$, the actual share of patients in group g who visit a hospital in system S. We describe this procedure in greater detail and explore robustness in Appendix A. In Panel B of Appendix Figure A.1, we show the distribution of ΔWTP_{eh}. The mean and median increases in WTP were 1.8% and 0.5%, respectively.[12]

In Panel C of Table 1, we analyze the post-merger price increases in our cohort of mergers, segmenting the transactions by the ΔWTP of the parties involved in the deals. Theory predicts that greater changes in WTP for a given hospital, or group of hospitals, will lead to greater price increases (Capps et al., 2003). Indeed, in Appendix Figure A.6, we generate a binned scatter plot of post-merger price increases against merger-driven changes in WTP, finding that the two are positively correlated. We flag mergers if they are estimated to raise WTP by 5% or more; 42 deals involving 82 hospitals are flagged by this measure. We find that flagged mergers increased composite prices by 3.6% (vs. 1.4% in our cohort with WTP increases of less than 5%). The WTP approach does better at predicting inpatient price increases, and we observe that hospitals with a WTP change of 5% or more raised their inpatient prices by 4.6% (we do not find a statistically significant change in outpatient prices). This is unsurprising given that WTP is estimated using demand for inpatient services.

5.3 The Margin for FTC Enforcement Actions

The two exercises above illustrate that there are many deals that can be *ex ante* predicted, via screening tools used by the FTC, to raise prices via a lessening of competition and observably do raise prices *ex post*. We view this as evidence of underenforcement of antitrust laws against hospital mergers. We view this as coming from low enforcement budgets, rather than from failures in *ex ante* merger screening methods. To demonstrate this, we compare the changes in HHI and WTP for cases that were litigated by the FTC against the changes in HHI and WTP for all the mergers in our sample

[12] In Appendix Figure A.2, we show a scatter plot of these changes against changes in HHI and illustrate that they are broadly correlated.

and mergers we flagged as potentially anticompetitive. Litigation typically focuses on the worst potential effects of the merger. To mimic this, we can take, for each transaction, the largest change across participating hospitals. As we illustrate in Appendix Table A.5, the changes in HHI and WTP for litigated cases are 3,607 and 22.9%, respectively. These cases where enforcement actions occurred involve changes in HHI and WTP that are markedly larger than the changes observed in our full sample of mergers (435 and 2.0%) or even in our flagged mergers (1,843 and 9.6%). This suggests that the FTC is able to identify problematic mergers, but highlights that their margin for intervention allows many anticompetitive mergers to be consummated.

6 Heterogeneity

In this section, we group mergers by the characteristics of the counties of participating hospitals, separating counties as a function of whether they were above or below the median for three measures: population density, wages per capita, and the share of the population in poverty.[13]

In Table 2, we show the average post-merger price increases for these sets of mergers. For each measure, the estimated average post-merger price increase was larger for mergers in less privileged or less densely populated regions, with differences of 2.8 percentage points (by share in poverty), 1.0 percentage points (by median income per capita, though not statistically significant), and 7.4 percentage points (by population density). In Appendix Figure A.7, we present the relationship between the post-merger price effects we observe and continuous measures of these characteristics.

A large share of the differences in Table 2 are a function of the relatively large increases in outpatient prices that mergers in less affluent areas generate. One potential explanation for this result is that these areas — due to low population density — have more concentrated markets for outpatient services. In these areas, mergers potentially give hospitals additional bargaining leverage over outpatient prices because there are fewer freestanding facilities (e.g., imaging or surgical centers) to constrain their price increases. Ultimately, markets for outpatient care are more local, and non-hospital outpatient facilities are more common in more densely populated areas due to economies of scale (Dingel et al., 2023). We confirm this in Appendix Figure A.8: less affluent areas, by all three measures, have fewer ambulatory surgical centers (non-hospital outpatient facilities) nearby. In Appendix Table A.6, we show that mergers involving hospitals in markets with fewer ambulatory surgical centers produce significantly larger outpatient price increases.[14]

[13]We measure county-level poverty from the American Community Survey's five-year estimates. Income per capita is measured as total county wages from the Quarterly Census of Wages and Employment divided by the total county population aged 25-64. Population density estimates are calculated as population per square mile, where county areas are measured using the Census Bureau's County and City Databook. County populations are measured using the Census Bureau's County Population Totals.

[14]This result is robust to alternative market definitions.

7 Discussion and Conclusion

We show that the 322 hospital mergers in our sample, on average, raised inpatient prices by 1.1%, outpatient prices by 1.8%, and total hospital prices by 1.6% within two years after being consummated. The massive wave of consolidation has led the US hospital industry to experience a preventable "death by a thousand cuts." Ultimately, an average year of mergers — approximately 53 transactions — raises health spending in following year by $204 million — larger than the entire antitrust enforcement budget of the FTC, despite the hospital sector only making up 6% of US GDP. Not only did this consolidation wave raise spending, it likely harmed equity as well, given that regions of the US with lower incomes faced greater price increases.

We conclude that there is likely too little antitrust enforcement in the US hospital sector. Our results suggest that existing pre-merger screening tools — both those that use simple market concentration measures and those that take a structural approach — can, *ex ante*, identify problematic mergers. In turn, these mergers generate large *ex post* price increases. Finally, the fact that we find post-merger outpatient price increases that are at least as large as inpatient price increases suggests that researchers and policymakers should consider the impact of mergers on outpatient prices during antitrust analysis, rather than largely focusing on the market for inpatient services.

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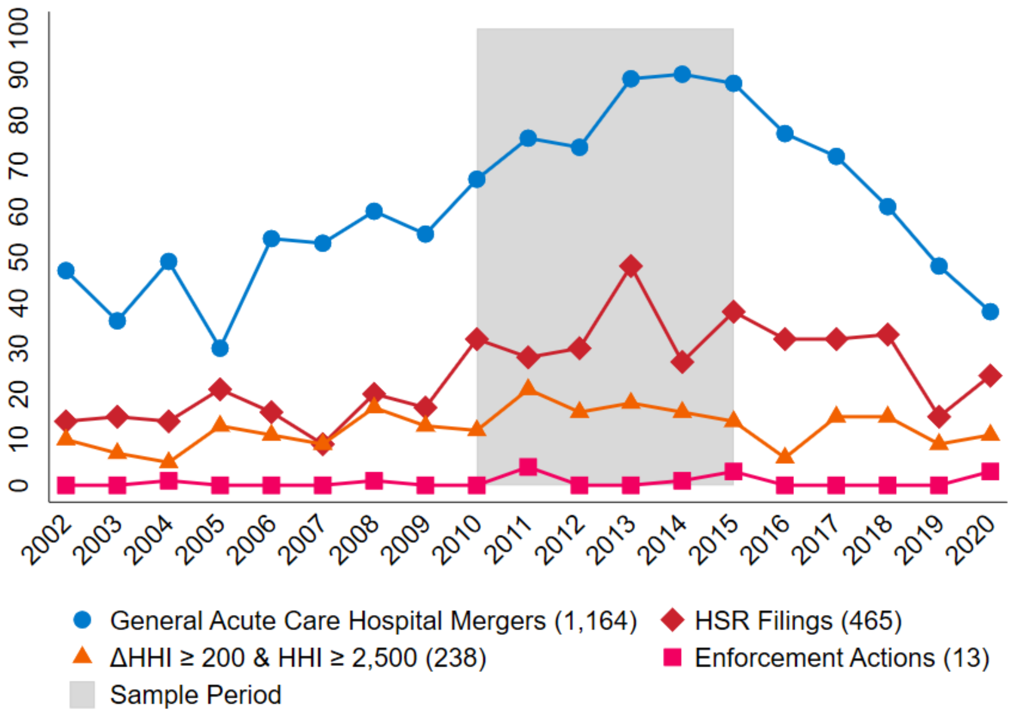
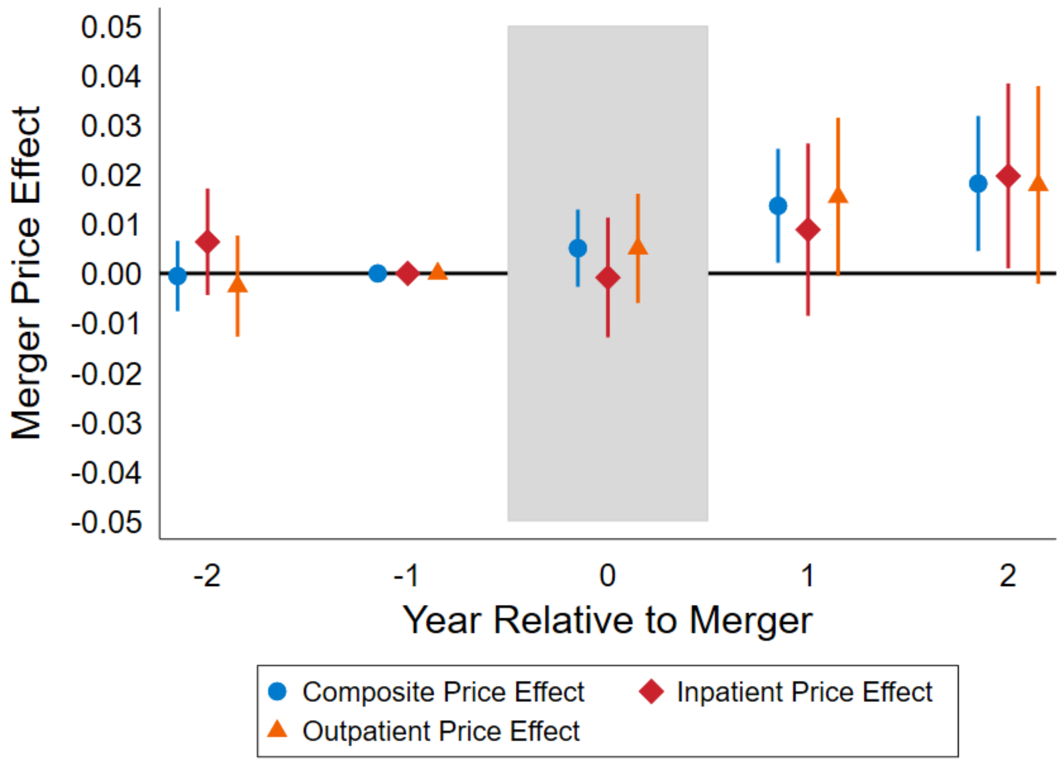
Figure 1: Hospital Mergers, HSR Filings, Presumptively Anticompetitive Mergers, and FTC Enforcement Actions by Year, 2002 to 2020



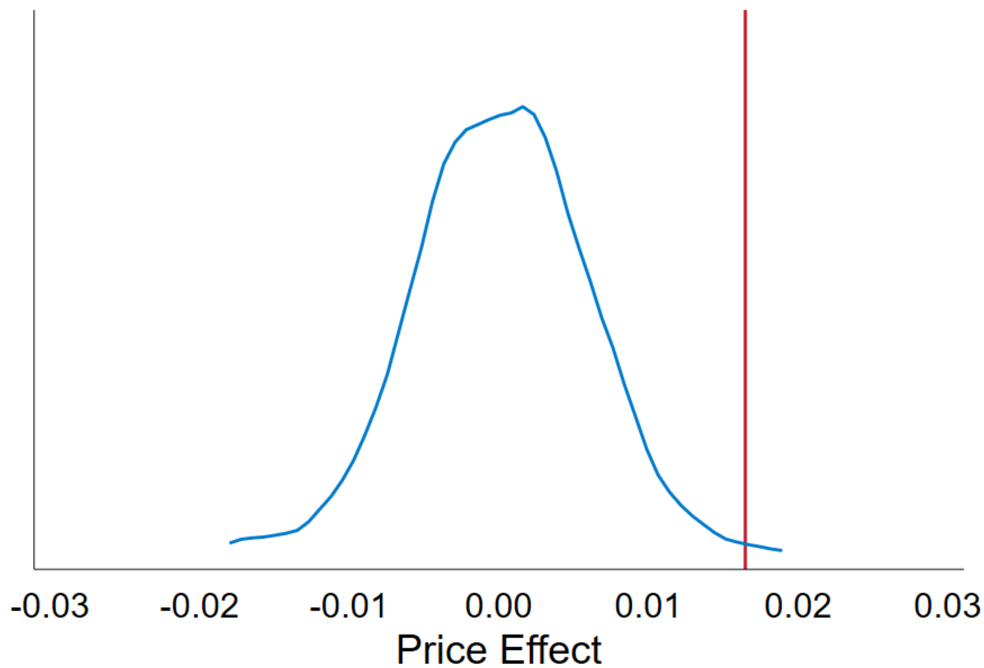
Note: The counts of mergers annually and mergers with an HHI increase of over 200 points that resulted in a post-merger HHI of over 2,500 are based on the authors' analysis. Data on HSR filings and FTC enforcement actions come from the FTC's Annual Reports to Congress Pursuant to the Hart-Scott-Rodino Antitrust Improvements Act of 1976. The HSR filings are reported in fiscal years; all other numbers are reported in calendar years. Enforcement actions are defined as matters that resulted in a final consent order requiring divestitures, matters where the parties abandoned or restructured the deal as a result of antitrust concerns raised during the investigation, or matters in which the FTC initiated litigation to block or undo the merger. The sample period used in our retrospective merger analysis is shaded in gray and spans from 2010 to 2015.

Figure 2: The Impact of Hospital Mergers on Inpatient, Outpatient, and Composite Hospital Prices



Note: This figure presents event study estimates of Equation (3) on our sample of 322 mergers involving 702 targets and acquirers located less than 50 miles from one another. Each dot represents a point estimate and the vertical line displays the corresponding 95% confidence interval. Hospital pricing data come from HCCI.

Figure 3: The Post-merger Price Increase from Mergers Relative to a Distribution of 1,000 Simulated Merger Cohorts



Note: This figure presents a distribution of average treatment effects for 1,000 placebo cohorts as described in Section 3. We estimate post-merger price effects as if control hospitals hadmerged, rather than actual merging hospitals. We then average these placebo estimates. We plot the kernel density of the distribution of average placebo post-merger effects on the composite price index (the blue curve) and the actual estimated average post-merger price effect (the red vertical line) on hospitals' composite prices. The x-axis is the price effect in log points. The share of placebo estimates above our actual estimates is 0.2%. We present the analogous results for our inpatient and outpatient price effects in Appendix Figure A.5.

Table 1: The Effect of Mergers on Hospital Prices

	Count of Hospitals (1)	Composite Price Effect (2)	Inpatient Price Effect (3)	Outpatient Price Effect (4)
Panel A: All Mergers				
Post-Merger Price Effect	702	0.016***	0.011**	0.018***
		(0.003)	(0.005)	(0.005)
Panel B: HHI				
$\Delta HHI \geq 200$ and Post-Merger HHI $\geq 2,500$	109	0.052***	0.054***	0.045***
		(0.008)	(0.011)	(0.011)
$\Delta HHI < 200$ or Post-merger HHI $< 2,500$	593	0.010***	0.004	0.013**
		(0.004)	(0.005)	(0.005)
Difference		0.042***	0.050***	0.032***
		(0.009)	(0.012)	(0.012)
Panel C: WTP				
$\Delta WTP \geq 5\%$	82	0.036***	0.046***	0.012
		(0.009)	(0.013)	(0.013)
$\Delta WTP < 5\%$	620	0.014***	0.007	0.019***
		(0.004)	(0.005)	(0.005)
Difference		0.022**	0.039***	–0.007
		(0.009)	(0.014)	(0.014)

Note: ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.* This table presents estimates from the regression given in Equation (3) on sub-samples of merging hospitals. The underlying regression is from a stacked difference-in-differences design comparing merging hospitals to a set of matched non-merging control hospitals before and after the merger of the focal hospital. Rows represent different sub-samples. Panel A reports the results for all mergers of hospitals within 50 miles of each other. Panel B compares merging hospitals with an HHI increase of over 200 points and a post-merger HHI greater than 2,500 points to merger hospitals with either an HHI increase less than 200 points or a post-merger HHI less than 2,500 points. For Panel B, a merging hospital's market is defined as all hospitals within a 30-minute drive time of the merging hospital, and market shares are defined using a hospital's share of inpatient beds in the market, measured using AHA data. Panel C segments merging hospitals by whether measured changes in willingness to pay as a result of their associated merger are above or below 5%. "Difference" denotes the difference in coefficients between the two sub-samples within the panel.

Table 2: The Effect of Mergers on Hospital Prices by Local Area Characteristics

	Count of Hospitals **(1)**	Composite Price Effect **(2)**	Inpatient Price Effect **(3)**	Outpatient Price Effect **(4)**
Panel A: Share Poverty				
Above Median Share Poverty	316	0.032***	0.024***	0.036***
		(0.006)	(0.008)	(0.008)
Below Median Share Poverty	386	0.004	0.001	0.004
		(0.004)	(0.005)	(0.006)
Difference		0.028***	0.023**	0.032***
		(0.007)	(0.010)	(0.010)
Panel B: Income per Capita				
Below Median Income per Capita	82	0.025*	–0.001	0.048***
		(0.013)	(0.021)	(0.017)
Above Median Income per Capita	620	0.015***	0.013***	0.014***
		(0.003)	(0.005)	(0.005)
Difference		0.010	–0.014	0.034*
		(0.013)	(0.021)	(0.018)
Panel C: Population Density				
Below Median Population Density	21	0.089***	0.063	0.108***
		(0.020)	(0.051)	(0.019)
Above Median Population Density	681	0.014***	0.010**	0.015***
		(0.003)	(0.005)	(0.005)
Difference		0.074***	0.053	0.093***
		(0.021)	(0.051)	(0.019)

Note: ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.* This table presents estimates from the regression given in Equation (3) on sub-samples of merging hospitals. The underlying regression is from a stacked difference-in-differences design comparing merging hospitals to a set of matched non-merging control hospitals before and after the merger of the focal hospital. Rows represent different sub-samples of mergers. Hospitals' local area characteristics are defined using the 2010 local area characteristic of the county each hospital is located in. Panel A segments merging hospitals by whether they are located in counties above or below median share poverty measured using the American Community Survey. Panel B reports the results by above- and below-median income per capita measured using the Quarterly Census on Employment and Wages. Panel C segments merging hospitals by above and below median population density measured using the Census's County and City Databook. The denominator for panels B and C is measured using the Census's County Population Totals files. Medians are calculated across all counties in the continental US. "Difference" denotes the difference in coefficients between the two sub-samples within the panel.

ONLINE APPENDICES

A Predicting Post-Merger Price Effects

Theory predicts that the extent to which mergers raise prices depends on the extent to which merging hospitals are good substitutes for one another, and whether their patients are unwilling to go to another hospital. As pointed out by Capps et al. (2003) and Gowrisankaran et al. (2015), since demand for hospitals is very inelastic, a standard model of Nash-Bertrand pricing would predict extremely high prices following mergers, and suggests mergers could raise prices by implausibly large amounts. Instead, these prior studies have developed a theory of price-setting in which prices are bilaterally negotiated between hospital systems and insurers, which bargain on behalf of their enrollees. In these models, prices are not determined by patients' price elasticities, but are instead driven by what is effectively the insurer's elasticity — in terms of how much insurers can subsequently raise premiums if the hospital system is included in the insurer's preferred network of hospitals. In this way, hospital prices are determined by patients' *ex ante* willingness to pay for the option to go to the hospital when buying an insurance plan (Ho and Lee, 2017, 2019).

A.1 Hospital-Insurer Bargaining and ΔWTP

Capps et al. (2003) model the *ex post* utility of patient i at hospital h as $U_{ih} = U(X_{ih}) + \varepsilon_{ih}$, where $U(\cdot)$ denotes expected utility at the hospital and ε_{ih} represents idiosyncratic patient preferences at specific hospitals with ε distributed i.i.d. standard Gumbel. X_{ih} contains patient and hospital characteristics that determine preferences for a given hospital, including the patient's specific health care needs as well as the distance between them and the hospital.

If a patient faces a hospital network $\mathscr{N}$ that limits what hospitals she has access to, then the patient's *ex ante* expected utility of access to a network $\mathscr{N}$ is

$$
\begin{aligned}
EU_i(\mathscr{N}) &= E[\max_{j\in\mathscr{N}} U_{ij}] \\
&= \log\left(\sum_{j\in\mathscr{N}} \exp(U_{ij})\right)
\end{aligned}
$$

Moreover, say that a hospital h is dropped from the network. Capps et al. (2003) show that the change in expected utility as a result of this network change is:

$$
\begin{aligned}
\Delta EU_{ih} &= EU_i(\mathscr{N}) - EU_i(\mathscr{N}\setminus h) \\
&= \log\left(\sum_{j\in\mathscr{N}} \exp(U_{ij})\right) - \log\left(\sum_{j\in\mathscr{N}\setminus h} \exp(U_{ij})\right) \\
&= \frac{1}{1-s_{ih}}
\end{aligned}
$$

where s_{ih} is that hospital's expected market share from patient i under network $\mathscr{N}$. If consumers are always indifferent between receiving a 1-point increase in EU and a \$$\gamma_i$ payment, then we can describe patients' *ex ante* "willingness-to-pay' for hospital h as $W_{ih} = \gamma_i \Delta EU_{ih}$. Both Capps et al.

(2003) and Gowrisankaran et al. (2015) show that, in standard models of bargaining (either pure Nash or Nash-in-Nash), the price for h's services that will be negotiated jointly by the hospital and insurer is proportional to W_h.

The above notation assumes that all hospitals are independent. If, instead, hospitals are part of some system S, the hospitals will bargain jointly. That is, prices will be determined by the willingness to pay for the *entire system*, $W_{iS} = \frac{1}{1-s_{iS}}$, with $s_{iS} = \sum_{j \in S} s_{ij}$. Systems are able to exert greater leverage than individual hospitals because they can threaten to hold out the entire system from the insurer's network if a deal on prices fails to be realized.[15]

We model the case of a hospital (h), which changes ownership from system S to system S' as a result of a merger event (e).[16] The change in bargaining leverage for that hospital ΔW_{eh} is equal to

$$\Delta W_{eh} = \int_i \gamma_i \left[\frac{1}{1 - s_{i,S'+h}} - \frac{1}{1 - s_{i,S+h}} \right] dF_i$$

Because W_{eh} is calculated by integrating over the distribution of consumers F_i, this quantity represents the amount that the average consumer is willing to pay for access to the entire system that contains h.

Due to a lack of data on individual insurance take-up, we follow Capps et al. (2003) and measure ΔW_{eh} under the assumption that $\gamma_i = \gamma$ for all patients. We go further by constructing the percent change in willingness to pay:

$$\Delta WTP_{eh} = \frac{\int_i \left[\frac{1}{1-s_{i,S'+h}} - \int_i \frac{1}{1-s_{i,S+h}} \right] dF_i}{\int_i \frac{1}{1-s_{i,S+h}} dF_i}$$

where γ_i drops out of the equation under the assumption of homogeneity.

Under these assumptions, the potential price changes due to a merger should be proportional to ΔWTP_{eh}.

A.2 Estimating Demand for Hospitals

Measuring ΔWTP_{eh} requires us to estimate substitution patterns in the relevant market. Capps et al. (2003) underscore the importance of patient heterogeneity in this calculation — heart attack patients may care much more about hospital closeness than patients undergoing elective surgeries.

We therefore take the semiparametric approach to demand estimation developed by Raval et al. (2017). That is, we estimate $U(X_{ih})$ by assuming we can partition patients into groups $g \in G$ based on their characteristics, such that

$$U_{ih} = U_{g(i)h} = \delta_{g(i)h} + \varepsilon_{ih}$$

.

Patients within the same groups are assumed to have the same *ex ante* expected utility for any

[15] In practice, we consider the relevant bargaining entity to be the system-HRR to avoid diffusing local changes in bargaining leverage over large acquiring systems. In unreported results, we consider the entire system and system-state to be the relevant bargaining unit and find that our results are not sensitive to this choice.

[16] This is without a loss of generality since system S could contain only hospital h prior to the merger.

particular hospital, but patients across groups may have different preferences in an unrestricted way. It is then true that, for patients within the same group, expected market shares at each hospital are equal within groups, such that:

$$s_{ih} = s_{g(i)h} = \frac{\exp(\delta_{g(i)h})}{\sum_{j \in \mathcal{N}} \exp(\delta_{g(i)j})}$$

Using this procedure, a valid partition of patients allows us to use the observed group-level market shares as an equivalent measure to individual-specific choice probabilities, and therefore patient utility for each hospital-by-group pair.

We calculate group-specific market shares for each hospital using every inpatient hospitalization for HCCI patients (in the group) during our relevant time period.[17] We exclude any hospitalization in which a patient attended a hospital more than 100 miles from their home. The Raval et al. (2017) approach provides an algorithm that partitions patients into increasingly small groups until the resulting groups are no smaller than S_{min}. This minimum group size parameter is set to balance a bias-variance trade-off: allowing for smaller groups reduces bias by allowing us to capture consumers' heterogeneous preferences for hospitals. However, smaller bins also increase variance by estimating preferences over smaller samples, where market shares may be estimated with error.

The algorithm proceeds as follows:

Step 1: The econometrician first establishes a set of discrete patient characteristics, ordered by "importance." Specifically, we group according to the following characteristics:

1. Patient home county
2. Patient home 5-digit zip code
3. Major Diagnostic Category of the patient's illness
4. Binary indicator for whether the patient's illness was such that the hospitalization was likely to be discretionary (rather than an emergency)
5. Binary indicator for whether the patient's illness was likely to require a surgical treatment (rather than a purely medical treatment)
6. Quartiles of the weight placed on the Diagnosis-Related Group for the patient's illness;[18]
7. The Diagnosis-Related Group for a patient's illness (as measured by their primary diagnosis code)
8. Patient age, in 10-year buckets
9. Patient sex

[17] That is, we assume that there is no relevant extensive margin substitution to no hospitalization as a result of changes in market structure.

[18] This DRG weight is used to determine hospital payments under Medicare's reimbursement system.

Step 2: We partition patients into groups based on their unique values for every characteristic (e.g., if the characteristics are gender, race, and county, there will be one group for black female patients in New York County, another group for white male patients in Cook County, etc.).

Step 3: We assign groups based on any partitions from Step 2, as long as the partition has a size above S_{min}. Any patients in partitions with size below S_{min} are left ungrouped.

Step 4: We then disregard the lowest-priority characteristic.

Step 5: We repeat Steps 2-4 until we reach a single characteristic (the patient's home county).

The partitions this algorithm produces vary in granularity to allow for more heterogeneity among patients characteristics when larger sample sizes are available. For example, denser counties will have more groups, subdivided by illness and patient demographics. By contrast all patients will be grouped together in smaller counties where data are sparser.

We run the algorithm separately for each year of mergers in our data. To ensure that we capture finer a finer partition of groups — and therefore flexible substitution patterns — we pool data from the two years prior for each year of mergers. We then compute patient choice probabilities for each hospital ($\hat{s}_{gh}$) for each group. To compute expected proportional changes in price, we compute the percent change in willingness-to-pay,

$$\Delta WTP_{eh} = \frac{\sum_g P_g \left[\frac{1}{1-s_{i,S'+h}} - \frac{1}{1-s_{i,S+h}}\right]}{\sum_g P_g \frac{1}{1-s_{i,S+h}}} \tag{4}$$

where P_g is the share of patients within group g.

B Addressing Staggered Timing Issues in the Difference-in-Difference Design

B.1 Matching Treated and Control Hospitals

Our primary approach to estimating our difference-in-difference is outlined in Section 3. In this section, we describe the procedure used to match treated hospitals to sets of matched comparison hospitals. We estimate a probit regression of the form:

$$\mathbb{P}\{Merger_h\} = X'\beta + \varepsilon_h \tag{5}$$

where X contains a vector of hospital characteristics — drawn from the AHA data and measured in the year before our first merger (2009) — that may meaningfully determine price trends at hospitals: total number of hospital beds; total inpatient admissions; full time equivalents; number of unique technologies; share of Medicare patients, share of Medicaid patients; whether the hospital is a teaching hospital; a non-profit hospital; or a government hospital; the distance to the hospital's nearest competitor; the distance to the hospital's nearest hospital in its system or not; and whether the hospital is independent or part of a system.

X also includes measures of local area characteristics around the hospital. First, we include the HHI, which is constructed as described in Section 2. Second, we include the share of the hospital's county covered by private insurance, which we construct using data from the Census's Small Area Health Insurance Estimates (SAHIE). Finally, we include the share of the county insured by HCCI payors specifically, using data from HCCI to form the numerator and data from SAHIE to form the denominator.

We then use the predicted values from Equation (5) as propensity scores. For each merging hospital, we find the 25 nearest neighbors that (1) never merge, or do not merge until after two years following the year of the focal merger, (2) have common support to the merging hospital in the price data from two years before and two years after the merger, and (3) are "close" to the merging hospital in propensity score space. We define close as within 20% of a standard deviation across all hospitals in the data.

B.2 Alternative Matching Approaches

In Table A.2, we present a series of robustness exercises aimed at testing sensitivity to our matching approach. In Panel A, we test alternative methods of calculating propensity scores using Mahalanobis distance and implementing the probit with a LASSO penalty. Because we include a long list of hospital characteristics in our probit, there is a risk that we might overfit the data when generating propensity scores. If this is a problem, a LASSO penalty would avoid this issue by restricting the number of coefficients admitted to the regression.[19] The resulting coefficients in Panel A are close in magnitude to our main estimates, and are significantly different from zero.

In Panel B of Table A.2, we re-estimate our treatment effects using alternative restrictions to our controls — allowing only five neighbors instead of 25, and omitting our caliper restriction on our main choice of 25 neighbors. Including only five neighbors weakens our precision on the inpatient

[19] The LASSO penalty results in the exclusion of the share of patients covered by Medicaid, and distance to the nearest hospital.

price effect, though the estimate is qualitatively similar to our main approach. All other estimates remain precise, and all estimates are qualitatively similar.

C Estimating Aggregate Spending Changes

In Section 4, we present estimates of the aggregate spending change generated by our sample of mergers. Our general approach is to multiply the price changes we observe by the *ex ante* level of commercial revenue at each hospital. We then sum across all mergers by year to obtain an aggregate spending estimate for each year of mergers from 2010 to 2015. To calculate the average 1-year spending increase from mergers, we take the average spending increase across the six years from 2010 to 2015. Formally, the mergers in our sample imply an average 1-year spending increase of

$$S = \sum_{t} \sum_{eh} \lambda_{eht} \times s_{eh,t-1} \tag{6}$$

λ_{eht} is the hospital-specific price effect estimated for "experiment" e in year t. $s_{eh,t-1}$ is the level of commercial spending at merging hospital *eh* in the year prior to its merger, which we estimate using data fro. The HCRIS provides information on hospital finances as a condition of hospitals' participation in the Medicare program. Although the HCRIS does not contain a direct measure of commercial revenue, it does include a measure of total revenue, as well as measures of charges (list prices) by payor type. We subtract Medicare and Medicaid charges from total charges to obtain an estimate of commercial charges. We then simulate the hospitals' average discount rate across all payors to obtain a multiplier that converts commercial charges into estimated commercial revenue.

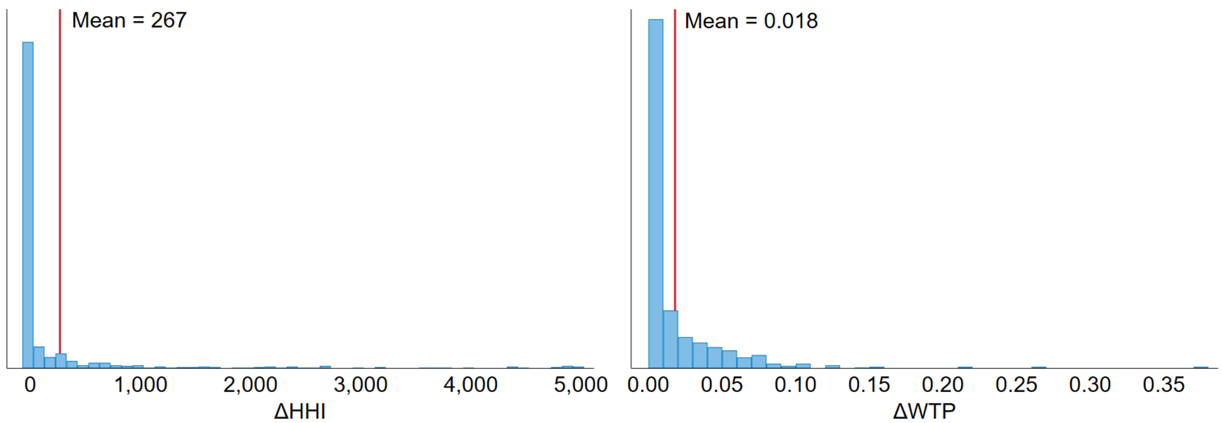
Note that using an overall average price-to-charge ratio to convert commercial charges to revenue likely understates the level of of commercial revenue because Medicare and Medicaid typically reimburse at levels much lower than commercial payors. We therefore regard our estimates of implied spending increases to be conservative, as actual commercial revenue is likely higher.

D Additional Tables and Figures

Figure A.1: Distributions of Change in HHI and Change in WTP for Hospital Mergers in Our Analytic Sample

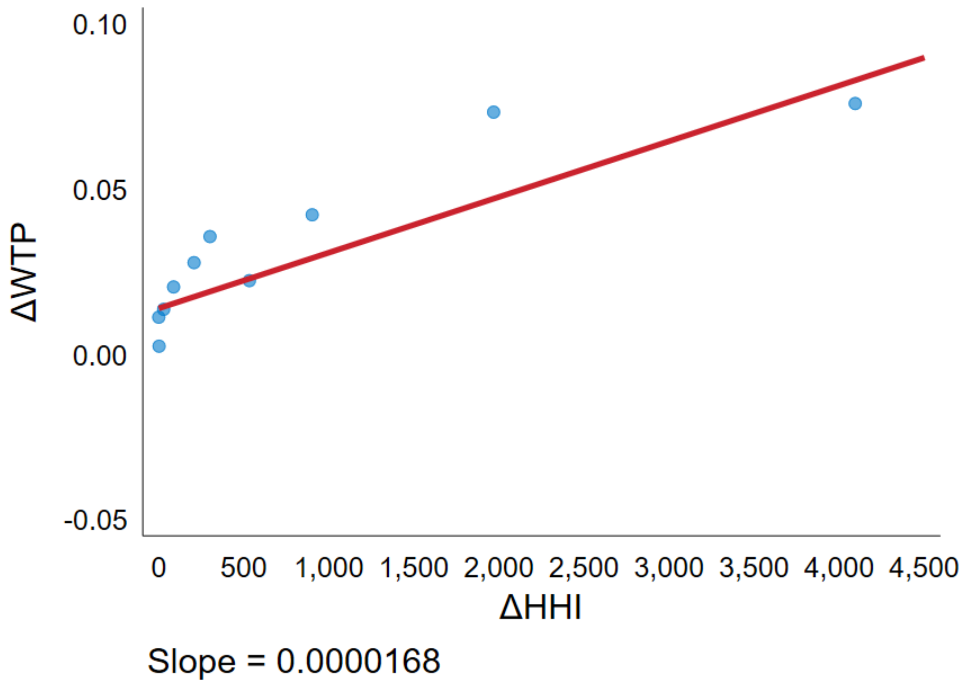
(a) Change in Hospital HHI per Merger

(b) Percent Change in Hospital WTP per Merger



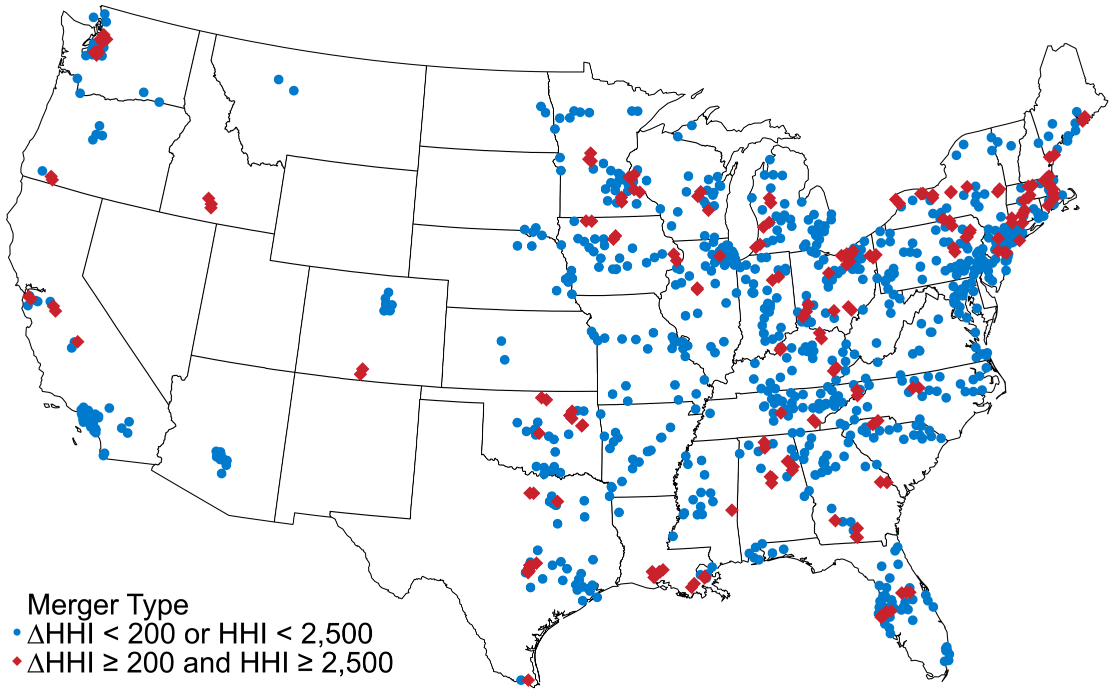
Note: This figure presents histograms of the distribution of changes in hospital HHI (Panel A) and percent changes in hospital WTP (Panel B, divided by 100) for each merging hospital. These figures are limited to the 702 merging hospitals in the analytic sample.

Figure A.2: Binned Scatterplot of Change in HHI and Percent Change in WTP



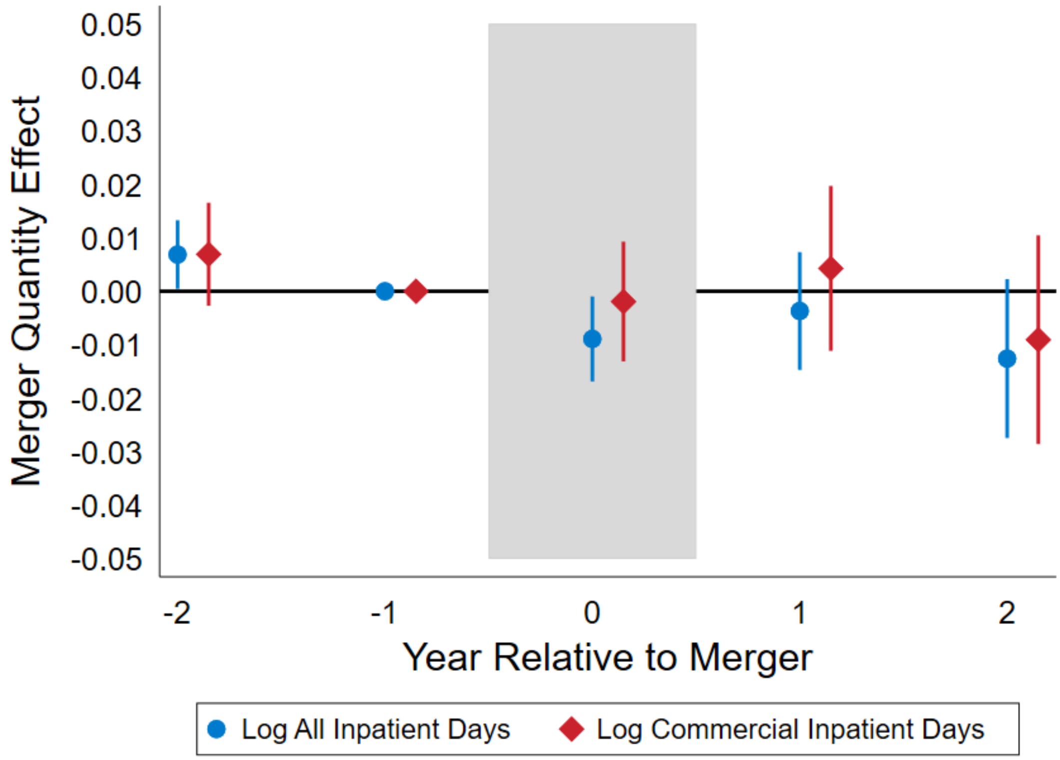
Note: This figure presents the relationship between ΔHHI and ΔWTP in a binned scatter plot. Each underlying observation is a merging hospital. The red line is the line of best fit.

Figure A.3: Hospital Mergers by Whether $\Delta HHI \geq 200$ and Post-Merger $HHI \geq 2,500$ From 2010-2015



Note: The map presents all hospital mergers in our analytic sample from 2010 to 2015. The market definition used to calculate HHI is a 30-minute drive time radius around each merging hospital.

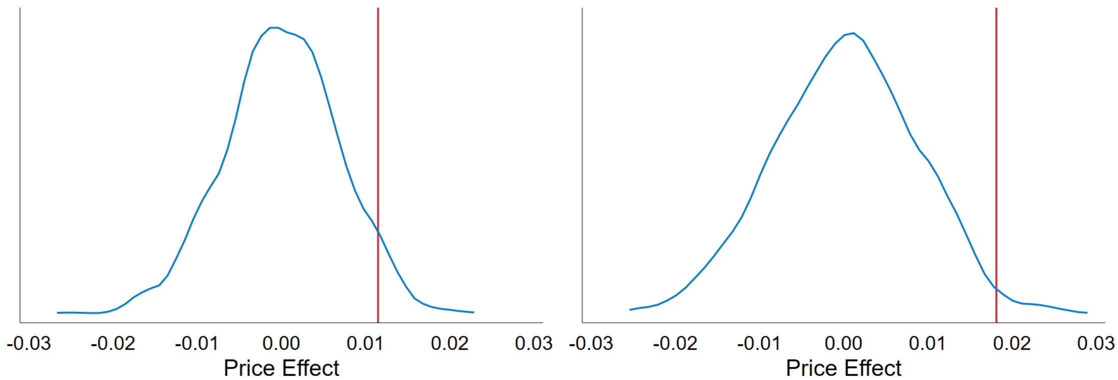
Figure A.4: The Impact of Hospital Mergers on All and Commercial Inpatient Days



Note: This figure shows event study estimates of total and commercial inpatient days for merging hospitals located within 50 miles of each other. The regression model used is similar to Equation (3), but the dependent variable is total inpatient days from HCRIS instead of HCCI prices. Each dot represents a point estimate, and the vertical line represents the corresponding 95% confidence interval. Commercial inpatient days are calculated as the difference between total inpatient days and the sum of Medicare and Medicaid inpatient days. To prevent duplication of inpatient days across hospitals with the same Medicare provider number, the analysis is limited to hospitals with a unique Medicare provider number.

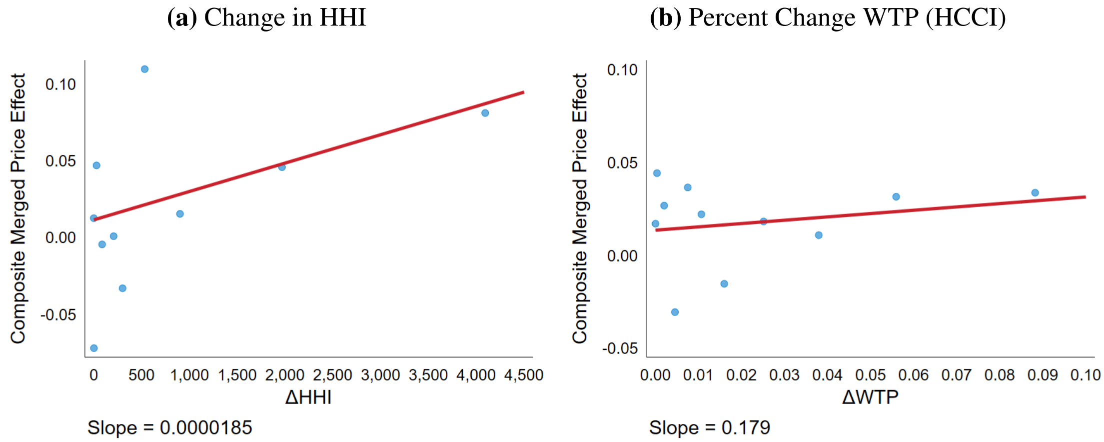
Figure A.5: The Post Merger Price Increase From Mergers Relative to a Placebo Distribution of Simulated Effects of 1,000 Mergers

(a) Inpatient Price Effect **(b)** Outpatient Price Effect



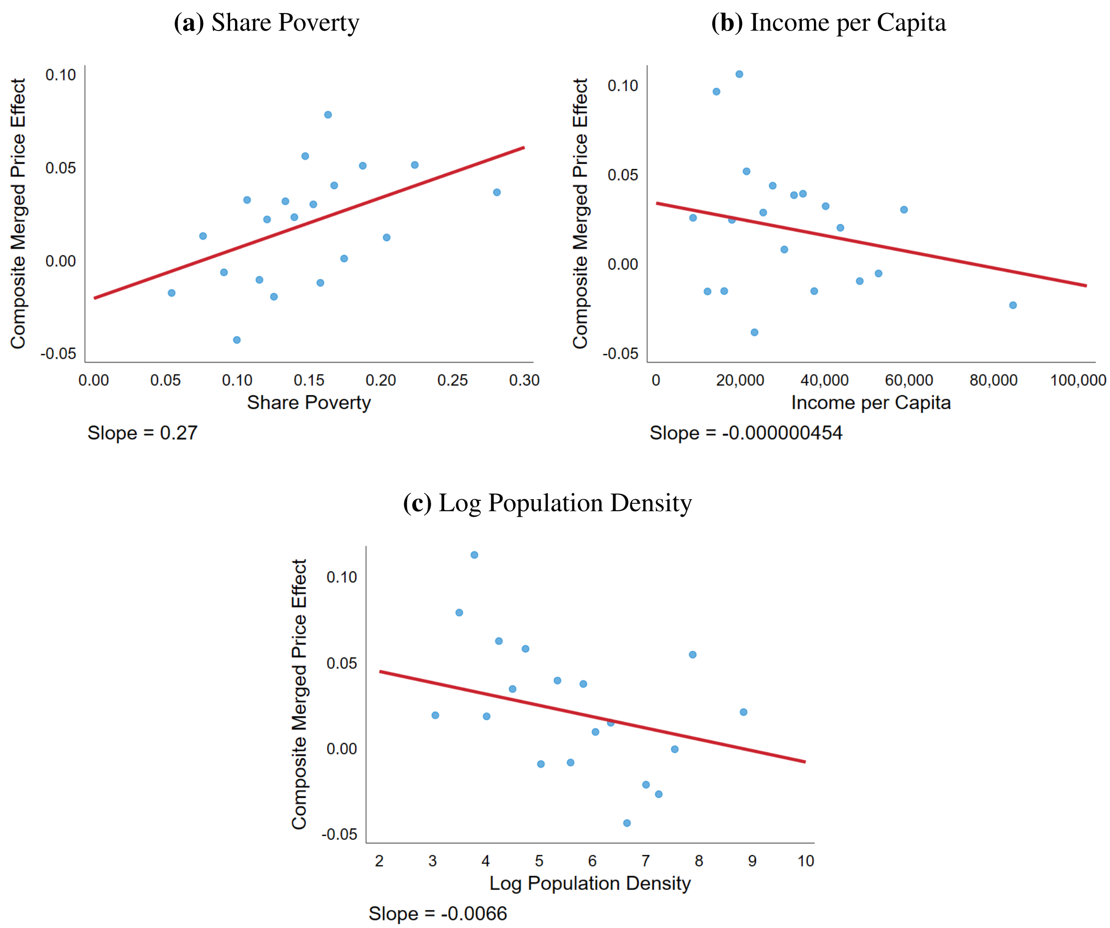
Note: This figure presents a distribution of average treatment effects for 1,000 placebo cohorts as described in Section 3. We estimate post-merger price effects as if control hospitals merged, rather than actual merging hospitals. We then average over these placebo estimates. We plot the kernel density of the distribution of average placebo post-merger effects on the composite price index (the blue curve) and the actual estimated average post-merger price effect (the red vertical line) on hospitals' composite prices. The x-axis is the price effect in log points. Panels (a) and (b) contain these results for inpatient and outpatient price indices, respectively.

Figure A.6: Binned Scatterplot of the Changes in Composite Price Effect and Change in HHI and Percent Change WTP



Note: This figure presents the relationship between our estimated post-merger price effect for the composite hospital price index and Delta HHI and WTP in a binned scatter plot. Each underlying observation is a merging hospital. The percent change in WTP is truncated at 10%. The red line is the line of best fit. Panel B presents the percent change in WTP estimated using HCCI inpatient claims data.

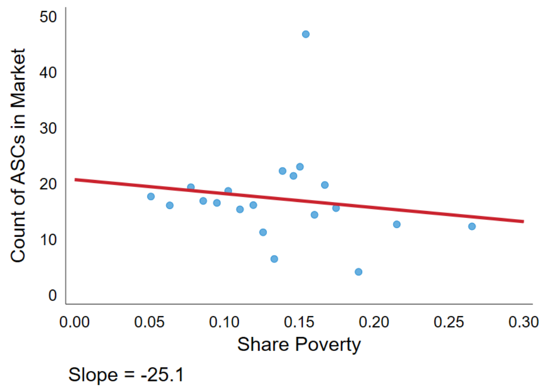
Figure A.7: Binned Scatter Plot of the Changes in the Composite Price Effect and Local Area Characteristics



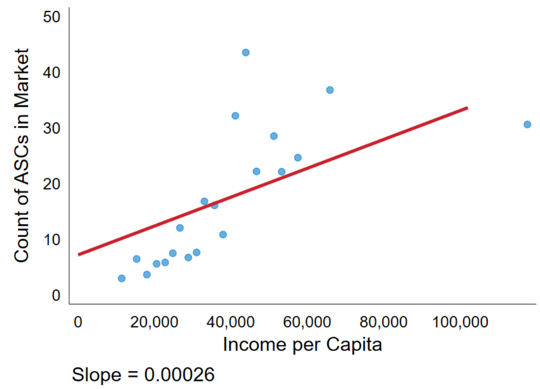
Note: This figure illustrates the relationship between the estimated post-merger price effect for the composite hospital price index and various local area characteristics. The underlying data are at the hospital level, and the local area characteristics of hospitals are determined based on the county in which each hospital is situated, using the 2010 data. County-level poverty is measured using the American Community Survey's 5-year estimates. Income per capita is calculated by dividing the total county wages from the Quarterly Census of Wages and Employment by the total county population aged 25-64. Population density estimates are determined by calculating the population per square mile, with county areas obtained from the Census Bureau's County and City Databook. County populations are measured using the Census Bureau's County Population Totals.

Figure A.8: Binned Scatterplot of the Number of ASCs in the Market and Local Area Characteristics

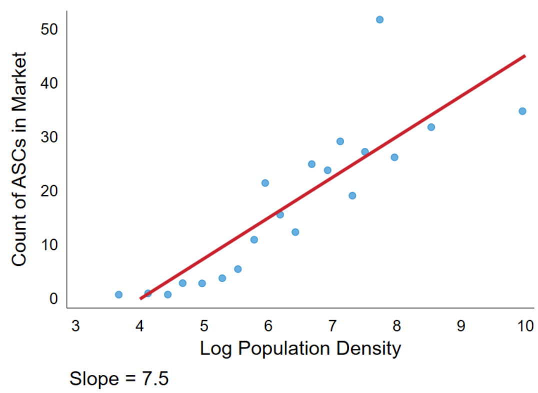
(a) Share Poverty



(b) Income per Capita



(c) Log Population Density



Note: This figure reports the relationship between the number of ASCs in the market and various local area characteristics. The underlying data are at the hospital level and are limited to the 702 merging hospitals in our analytic sample. The number of ASCs in the market is defined as all ASCs within a 30-minute drive time of the merging hospital, using the 2010 Medicare Provider of Services file. ASC locations are determined based on the centroid of their zip code. The local area characteristics of hospitals are determined based on the county in which each hospital is situated, using the 2010 data. County-level poverty is measured using the American Community Survey's 5-year estimates. Income per capita is calculated by dividing the total county wages from the Quarterly Census of Wages and Employment by the total county population aged 25-64. Population density estimates are determined by calculating the population per square mile, with county areas obtained from the Census Bureau's County and City Databook. County populations are measured using the Census Bureau's County Population Totals.

Table A.1: Merger Characteristics

	All 2002-2020 (1)	All 2010-2015 (2)	50 Mile 2010-2015 (3)	In Estimation 2010-2015 (4)
Number of Transactions	1,164	484	377	322
Average Number of Acquirer Hospitals	18.1	18.1	18.0	18.7
Average Number of Target Hospitals	1.6	1.5	1.4	1.5
Share $\Delta HHI \geq 200$ and Post-Merger $HHI \geq 2,500$	20.4%	20.0%	25.7%	25.5%
Share $\Delta WTP \geq 5\%$		9.3%	11.7%	13.0%

Note: This table presents summary statistics hospital mergers under various sample restrictions. Column (1) presents all hospital mergers occurring between 2002 and 2020. Column (2) restricts to mergers occurring between 2010 and 2015. In Column (3) we focus on the subset of these mergers where at least two of the prior competitor hospitals were located within 50 miles of one another. Column (4) shows how the sample changes when we restrict to the subset of merging hospitals in Column (3) for which we have sufficient data from HCCI to estimate our difference-in-difference model.

Table A.2: Robustness to Alternative Matching Algorithms and Matching Specifications

	Count of Hospitals (1)	Composite Price Effect (2)	Inpatient Price Effect (3)	Outpatient Price Effect (4)
Panel A: Alternative Matching Algorithms				
Probit	702	0.016***	0.011**	0.018***
		(0.003)	(0.005)	(0.005)
LASSO Probit	702	0.016***	0.010**	0.019***
		(0.003)	(0.005)	(0.005)
Mahalanobis	702	0.021***	0.012**	0.027***
		(0.003)	(0.005)	(0.005)
Panel B: Alternative Matching Specifications				
25 Neighbors, 20% Caliper	702	0.016***	0.011**	0.018***
		(0.003)	(0.005)	(0.005)
5 Neighbors, 20% Caliper	702	0.016***	0.009	0.019***
		(0.005)	(0.006)	(0.007)
25 Neighbors, No Caliper	702	0.016***	0.012**	0.018***
		(0.003)	(0.005)	(0.005)

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents estimates from the regression given in Equation (3) on various matching specifications. The first row in each panel is our baseline specification, which limits to merging hospitals within 50 miles of a former rival hospital in the merging system. The baseline specification also uses probit regression to estimate the propensity scores, and selects the 25 nearest hospitals as controls (25 neighbors) within 0.2 times the standard deviation of the propensity scores (20 calipers). Panel A presents results using a Mahalanobis distance instead of a probit regression or using LASSO regularization to limit the characteristics that enter the match. Panel B varies the number of nearest neighbors selected and the caliper restriction.

Table A.3: Robustness to Alternative Maximum Distances Between Merging Parties

	Count of Hospitals **(1)**	Composite Price Effect **(2)**	Inpatient Price Effect **(3)**	Outpatient Price Effect **(4)**
50 Miles	702	0.016***	0.011**	0.018***
		(0.003)	(0.005)	(0.005)
400 Miles	949	0.013***	0.014***	0.011***
		(0.003)	(0.004)	(0.004)

Note: ** p < 0.1, ** p < 0.05 , *** p < 0.01*. This table presents estimates from the regression given in Equation (3) on various maximum distances between merging parties. The first row is our baseline specification, which limits to merging hospitals within 50 miles of a former rival hospital in the merging system. The second row shows the overall effect of mergers on hospital prices when including additional merging hospitals over 50 miles away from their closest, former rival hospital in the merging system.

Table A.4: Merger Price Effects For Deals Above and Below ΔHHI Thresholds Calculated Using Various Market Definitions HHI

	Count of Hospitals (1)	Composite Price Effect (2)	Inpatient Price Effect (3)	Outpatient Price Effect (4)
Panel A: 30-Minute Drive Time Radius				
$\Delta HHI \geq 200$ and Post-Merger $HHI \geq 2,500$	109	0.052***	0.054***	0.045***
		(0.008)	(0.011)	(0.011)
$\Delta HHI < 200$ or Post-Merger $HHI < 2,500$	593	0.010***	0.004	0.013**
		(0.004)	(0.005)	(0.005)
Difference		0.042***	0.050***	0.032***
		(0.009)	(0.012)	(0.012)
Panel B: 15-Mile Radius				
$\Delta HHI \geq 200$ and Post-Merger HHI $\geq 2,500$	112	0.028***	0.043***	0.013
		(0.007)	(0.009)	(0.011)
$\Delta HHI < 200$ or Post-Merger HHI $< 2,500$	590	0.014***	0.006	0.019***
		(0.004)	(0.005)	(0.005)
Difference		0.014*	0.036***	–0.006
		(0.008)	(0.011)	(0.012)

Note: ** p < 0.1, ** p < 0.05 , *** p < 0.01*. This table presents estimates from the regression given in Equation (3) on various market definitions to define HHI. The underlying regression is from a stacked difference-in-differences design comparing merging hospitals to a set of matched non-merging control hospitals before and after the merger of the focal hospital. Each panel defines HHI using a different market definition. Panel A defines the market as including all hospitals within a 30 minute drive time of the focal hospital. Panel B defines the market as including all hospitals within a 15 miles of the focal hospital. Market shares are defined using a hospital's share of inpatient beds in the market, measured using AHA data. "Difference" denotes the difference in coefficients between the two sub-samples within the panel.

Table A.5: Changes in Concentration and Competition for FTC-Litigated Mergers Compared to Consummated Mergers

	Full Sample	Flagged Mergers	FTC Enforced
Change in Concentration (HHI)	435	1,843	3,607
Change in Concentration (WTP)	2.0%	9.6%	22.9%

Note: This table presents estimates of the changes in the Herfindahl-Hirshman Index (HHI) and willingness-to-pay (WTP) for three sets of mergers: 1) The full sample of mergers in our sample, 2) The set of mergers flagged by our premerger screening approach (for HHI, mergers that changed HHI by at least 200 points and resulted in a post-merger HHI of at least 2500 points; for WTP, mergers that resulted in an estimated change in WTP of at least 5%), and 3) Mergers that the FTC took an enforcement action against during 2010-2015. For each transaction in the category, we take the maximum change in HHI/WTP across hospitals within the transaction, then average across transactions.

Table A.6: Merger Price Effects by Count of Ambulatory Surgical Centers (ASCs) in the Market

	Count of Hospitals (1)	Composite Price Effect (2)	Inpatient Price Effect (3)	Outpatient Price Effect (4)
Panel A: 30-Minute Drive Time				
Below Median ASCs in Market	327	0.027***	0.002	0.043***
		(0.005)	(0.008)	(0.007)
Above Median ASCs in Market	375	0.007	0.020***	–0.004
		(0.004)	(0.006)	(0.007)
Difference		0.019***	–0.018*	0.047***
		(0.007)	(0.010)	(0.010)
Panel B: 15-Mile Radius				
Below Median ASCs in Market	245	0.024***	0.001	0.035***
		(0.007)	(0.010)	(0.008)
Above Median ASCs in Market	457	0.012***	0.017***	0.009
		(0.004)	(0.005)	(0.006)
Difference		0.012	–0.016	0.026**
		(0.008)	(0.011)	(0.010)

Note: ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$*. This table presents estimates from the regression given in Equation (3) subset to hospitals with different counts of ambulatory surgical centers in their local market. Panel A defines the relevant market as all ASCs within a 30 minute drive time of the focal hospital. Panel B defines the relevant market as all ASCs within 15 miles of the focal hospital. The number of ASCs is determined using the 2010 Medicare Provider of Services File and the location of ASCs is set as its zip code centroid.